



DEPARTMENT OF  
HEALTH, EDUCATION AND WELFARE  
~~FEDERAL SECURITY AGENCY~~  
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

Communicable Disease Center,  
Enteric Bacteriology Laboratories  
P. O. Box 185  
Chamblee, Georgia

June 22, 1953

Dr. Joshua Lederberg  
University of Wisconsin  
Madison 6, Wisconsin

Dear Dr. Lederberg:

Having returned from leave I shall try to answer your letters of June 2 and 8 in an orderly manner and report on a few points not previously mentioned. As you remarked, our correspondence has been rather hectic in nature.

1. The edited manuscript is returned herewith.

*Take or leave  
the others  
as you  
like!*

I was pleased to find that no serious editorial changes were made. There were a few grammatical changes which probably were justified but which we both missed. Probably the editor of the journal also will make these and will designate his degrees C, etc. as he wants them. The only important changes involve nomenclature of CDC, the department, etc. Since the people here are rather particular about the form of these references to the institution, I would appreciate your making the indicated changes. I shall make the agreed change on page 9 and write a substitute statement for page 12. Perhaps you can modify the latter to make it suitable. The thought contained in this passage is a difficult one to express.

2. The strain histories have been received. Many thanks.

3. Some time ago you raised a question about the presence of V antigen in SW 998 as evidence against the transfer of V to SW 1003. The answer is that SW 998 ~~does not~~ have V and was recorded as IV,XII on our original work sheets. The V was an error in transcription. I have gone over this matter thoroughly and can assure you that V is present in SW 1003 but not in SW 998. I spoke of the transfer of phase variation to SW 1003 and I believe this is correct but further work is required. Most of the a forms which have come out of S. abortus equi show a reluctance to change in phase or go only a certain distance, as your a-b or i-b forms. I believe SW 1003 is different in this respect and is truly diphasic. As soon as I crawl out from under the accumulated back log of paper, I will look into this.

Dr. J. Lederberg

June 22, 1953

One other fact has come out of this IV,XII and IV,V,XII work. V antigen does not act as a masking antigen for O agglutination as intimated by Kauffmann and stated by Felix. The differences observed in agglutinin titers are merely a reflection of the interaction of partial antigens, i.e. whether an agglutinin hits an antigen on the nose or merely plays around the edges. It is true that a IV,XII serum will not agglutinate a IV,V,XII antigen to titer (as emphasized by Felix) but it is also true that a IV,V,XII serum will not agglutinate a IV,XII antigen to titer. Do I make myself clear? To me, this is much more important than the presence or absence of antigen I, which is subject to form variation. I would not worry about the latter. I still believe that if I can make SW 1003 behave as regards phase variation that these points justify a brief communication. What do you think?

4. I have given up on the i-b forms (SW 1026) with which I could only duplicate your results but must look farther at SW 1031 (a-b). I have not done the work on the latter personally but will take it over. I think I should also look at SW 1052 and SW 1053. When I shall do these things I cannot say since I must go to the West coast on business next month. At least it is pleasant to contemplate the work! Incidentally, you are seriously disturbing my ideas of phase variation. I do not understand your results with N 97 colonies but they are most intriguing.

5. The shipment of cultures mentioned in your letter of June 8 will be examined at earliest possible moment.

6. Thanks for the Iseki reprints. I will return them at an early date but probably will do nothing about the group E transformation. Bruner tells me he has changed the subgroups within group C.

7. I agree to the change in regard to the XII<sub>2</sub> component acting as a phage receptor. In fact, I am happier about it.

8. I agree to the changes in SW 970 and SW 972. They are undoubtedly the same culture.

9. As stated above I believe the I antigen is not worth working on since it is known to be subject to form variation.

10. In view of the several changes in the manuscript should you not try to get it back before the type is set up? Perhaps the editor would permit you to make these changes before sending the paper to the publisher.

11. I shall send the serums requested as well as b and e,n,x serums from S. minnesota. The factor V serum must be absorbed and will be along later.

Dr. J. Lederberg

June 22, 1953

I believe the above covers most points under discussion.

With best wishes, I am

For the Officer-in-Charge, Bacteriology Section

Sincerely yours,

*Phil*

PRE:mg  
Encl.

Philip R. Edwards, Ph. D.  
Bacteriologist-in-Charge  
Enteric Bacteriology Unit

P.S.

Page 12. Agglutinin Absorptions. The first sentence should remain unchanged. The second sentence is unnecessary since the thought is carried by the first sentence. In place of the second sentence I would substitute "These results are in direct contrast to changes produced within a single antigenic complex by cultivation in agglutinating serum (7, 26, 27)."

26. Bruner, D. W. and Edwards, P. R. J. Bact., 55: 137, 1948.  
(See reprint)

27. Bruner, D. W. J. Bact., 57: 388, 1949.

I would omit the third sentence since I think it adds nothing.

You may follow your judgment in regard to these changes.

*Joshua - The Isletti work seems straightforward enough but I am still doubtful. I would like to repeat the work but when will I find the time? More & more I become an exec and get nothing done. There is no one to whom I can assign this work at the moment.*

*Hope you are having a good summer  
My ever most pleasant regards etc.*

*E.*